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DISCUSSION PAPER 25

Explaining the geography of income redistribution in Belgium: the effect of political representation

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Explaining the geography of income redistribution in Belgium: the effect of political representation[1]

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Abstract:

This paper investigates the role of political representation in explaining geographical variation in social security and income tax transfers in Belgium. The transfers we consider are the net amount of federal income taxes and social security contributions paid and in-cash social benefits received. These make up the core of the Belgian welfare state and income redistribution. For the 1995-2010 period, we find that these transfers to inhabitants of a particular Belgian electoral district increase significantly with every extra federal minister originating from that electoral district. Given that such transfers are largely formula-based, and if anything would be easier to target across social groups rather than geographically, this result is surprising. Nevertheless, the result is robust to controlling for economic and demographic variables that are important determinants of transfers, i.e. (gross) income per capita and the share of the unemployed, the young and the retired, as well as to controlling for the share of parliamentary seats of the governing coalition per electoral district. The observed correlation between political representation and transfers is suggestive of a system with simultaneous causation, with politicians steering transfers to their constituencies in the hope to gain votes; and where voters in turn reward politicians in the hope to gain transfers. To isolate and quantify the causal link going from political representation to transfers, we identify two sets of exogenous changes in political representation and use an instrumental variable approach. A first set considers changes in political representation due to ministers having to resign, mainly due to political crises and scandals. A second set considers changes in the political representation due to changes in the borders of electoral districts in 2002.

[1] Our paper has greatly benefitted from critical comments and suggestions from Thushyanthan Baskaran, Andreas Bernecker, Liesbeth Colen, Matz Dahlberg, Klaus Desmet, Bart Maddens, Kevin Milligan, Panu Poutvaara, Jørn Rattsø, Michael Smart, Marc Swyngedouw, Philippe Van Parijs, participants at the 2013 EPCS Conference in Zuerich and the 2013 IIPF Conference in Taormina, and our colleagues at VIVES and elsewhere. We thank the National Bank of Belgium, the Belgian federal Ministries of Pensions and of Employment, Statistics Belgium, Jo Buelens (VUB) and the federal Parliament of Belgium for data provision.

1 Introduction

This paper investigates the role of political variables in explaining geographical variation in social security and personal income tax transfers in Belgium. In a simple fixed effects specification using data on income and transfers collected by the central bank of Belgium (NBB) for the years 1995 to 2010, we find that cash transfers to an electoral district are 21 euros higher per capita and year, for every federal minister originating from that electoral district. This result is robust to controlling for local economic and demographic variables such as the local share of retired, minors and unemployed in the population. This observed correlation does not prove or quantify a causal link going from political representation to increasing transfers to an electoral district. We rather consider this result to be thought provoking, as such transfers are typically formula based, and suggestive of a mechanism of mutual causation, where government executives cater for their electorate using cash transfers in the hope of being rewarded with re-election.

Although we believe that the observed correlation between representation in the executive and transfers is an interesting result in itself, we subsequently attempt to isolate the causal effect of a change in political representation on transfers by considering purely exogenous changes in executive representation and use this information in an instrumental variable analysis.

Our paper is most closely related to Crampton (2004) and Milligan and Smart (2005) for Canada and to Golden and Picci (2008) for Italy. In these contributions, the distribution of discretionary grants over electoral districts is explained by the number of cabinet ministers per electoral district and by the number of parliamentary seats won by the government. Other closely related work is Evans (2005) and Jutras and Vaillancourt (2008), who explain the distribution of formula-based fiscal transfers resulting from the income tax and the social security system over Canadian electoral districts by the total number of available parliamentary seats per province.

Belgium is a federal country “sui generis” in that its political parties are split along linguistic lines and –leaving aside the electoral district of Brussel-Halle-Vilvoorde- along geographical lines. Since the 1970s there has not been a federal member of parliament who obtained votes in both main communities of the country. At the same time the income disparities between the southern French speaking Walloon Region and the northern Dutch speaking Flemish Region have been large and persistent. In combination with a progressive federal tax system, a high federal tax burden, and a large social budget, these income disparities are among the causes of the considerable and persistent net fiscal transfers between the 3 Regions in Belgium, with the Flemish region as a net payer and the Walloon Region a net receiver; and the central Brussels Capital Region showing either small outgoing or incoming net fiscal transfers according to most studies.

In Belgium, as in many countries, the executive dominates the legislative, and cabinet ministers do so prominently. When looking for a political explanation of the geographical distribution of fiscal transfers in Belgium, we are therefore inclined to focus on the geographical distribution of federal cabinet ministers. Although the

French speaking community only accounts for some 40 percent of the total Belgian population, the number of federal government ministers from both main linguistic communities is equal by law. Any attempt to explain changes in the amount of transfers between Regions by changes in the number of government ministers from the Flemish and the Francophone communities would therefore be futile. We hence rather opt to explain the distribution of federal fiscal transfers between the 20 Belgian electoral districts into which Belgium was divided on the occasion of the 1995 and 1999 federal elections for the House of Representatives.

In 2002 a law was voted merging these 20 districts into 11 larger "provincial" electoral districts[2]. The maps in Annex 3 show the original and the new electoral districts, together with their respective sizes in terms of total parliamentary seats to win. The re-shuffling of electoral districts poses both a practical problem and an opportunity for the analysis. In the first part of our analysis, we will ignore the change in electoral districts and make the assumption that also during the latter half of our sample period federal ministers kept on targeting mainly their smaller "old" electoral districts, even after the "provincial" electoral districts had come into effect[3]. In the second part, we fully take into account the change in the borders of electoral districts, and use this change to isolate changes in political representation which are exogenous to current or previous transfers. These exogenous changes will be used to identify causally how political representation in the executive affects transfers using an instrumental variable analysis. A separate IV setup utilises changes in political representation which are due to ministers having to resign, mainly due to political crises and scandals, as instruments.

We do not know of previous attempts to explain the geographical distribution of fiscal transfers in Belgium by means of a combination of political, demographic and economic variables[4]. There exists anecdotal evidence of political room for manoeuvre in allocating expenditures geographically. One example is the recent accusation of "pork barrel" politics by both the prime minister and the minister of economy in the di Rupo government (Le Vif / L'Express 2012, and Pauwels and Van

[2] With the notable exceptions of the old electoral districts of Brussel-Halle-Vilvoorde and of Leuven, which were kept into existence. So after the 2002 reform, Belgium was left with 11 electoral districts, of which 9 truly provincial.

[3] Golden and Picci (2008 p. 272) make a similar assumption. Saarimaa and Tukiainen (2013) find evidence for Finland that voters keep voting for political representatives living in their original, smaller electoral districts to the same extent after the merging of these smaller districts into a larger electoral district.

As a robustness check, we have re-conducted our analysis below while assuming the 11 "provincial" electoral districts held during our entire sample period. This assumption does not affect our main findings, although the lower number of observations reduces the precision of the estimates in a number of analyses.

Another complicating factor is the (small) number of ministers who had been elected for the Senate over our sample period. Their number is on average less than 2 per year over our sample period, out of a total of 14 to 15 ministers per year. As opposed to the House of Representatives, the Senate is characterized by only 2 electoral districts, i.e. the Flemish community and the French-speaking community. However, we assume that also ministers elected for the Senate would predominantly target the electoral district which would be theirs if they would have run for the House of Representatives, i.e. the House's electoral district in which they reside. Reasons are that the geographical scope of a minister's reelection efforts seems naturally limited and that in Belgium members of parliament are not sure for which part of parliament they will be requested by their party to run during the next elections, the House of Representatives or the Senate.

[4] Studies explaining the geographical distribution of fiscal transfers in Belgium that did not control for political factors are Deleeck e.a. (1989), and its update by Cantillon and De Maesschalck (2007).

den Eynde 2012). But while "pork barrel" traditionally refers to *discretionary investment* expenditures, in this paper we look at the geographical distribution of the income tax and social security contributions and expenditures, which are two important *transfer* categories which are firmly based on legislation, and therefore –presumably- *formula-based*. The social expenditures we consider comprise all major social transfer categories in Belgium. In decreasing order of financial weight, these are pensions, unemployment benefits, child allowances, work disability allowances, allowances for the handicapped, and subsistence allowances[5]. In the following paragraphs, we give some examples of how there might exist some scope for geographical tailoring within these types of transfers.

A first example of how ministers could cater for their constituency is the creation of ever new target group measures in social security (e.g. employment schemes and rebates in social security contributions) and in the tax system (e.g. targeted tax rebates), although it is easier to imagine such tailoring to take place on a larger Regional level than on a smaller electoral district level. There are dozens of such targeted measures, some of which have led to criticism precisely because of their asymmetric regional impact (for example the so-called `Rosetta' measures). Ironically, attempts to simplify the federal legislation have equally been criticised because of their regional impact, and some proposals have been blocked in 2009 by the Flemish regional parliament to force renegotiations on the federal level.

An alternative way for politicians to steer transfers to an area as small as their electoral district, might operate not through the *creation* of new legislation related to income taxation and social security, but through the *(non-)application* of existing legislation. E.g. politicians may prod local branches of the tax and social security administrations towards lenient taxation and social security audits. Indeed, considerable geographical disparity in the number of tax and social security audits has been found to exist[6] (Deloitte 2010).

Further scope for geographical tailoring is offered by the fact that the agreement of the federal minister of employment is required to secure exceptional state-funded early retirement for its employees for any firm that has gone out of business. The starting age of eligibility for such early retirement appears to be decided case by case. This age appears to vary between 50 and 58 depending on the negotiation outcome.

Eligibility for a number of other social benefits and the particular amount of them inevitably depends upon a judgement call, all the more so in Belgium as the related legislation is complicated. This holds in particular for sickness and work disability allowances. Over the last 10 years, the number of beneficiaries of a sickness or a work disability allowance in Belgium has steeply increased. The press has reported that such allowances have increasingly been used by the federal government as a complement to –sometimes meagre- retirement benefits. However, beneficiaries have to formally request such a complement before receiving it, requiring decisions

[5] The latter 2 categories are not formally part of the Belgian social security system, i.e. they are 100% funded out of tax revenues other than social security contributions.

[6] In Belgium there exists quite some anecdotal evidence on pervasive fraud and evasion possibilities with respect to the payment of the personal income tax and of social security contributions, as well as with respect to the benefiting of social allowances.

at the level of the ministry. Similarly, the Ministry of Social Affairs has to approve individual requests by self-employed who wish to have the payment of their social security contributions waived or postponed because of the economic crisis. Also subsistence allowances are approved on a case by case basis depending on means-testing of the claimant.

In Belgium the federal government decides unanimously on policy and proposals for new legislation through a council of all ministers which meets regularly. This setting offers plenty of scope for negotiation and bargaining among ministers, and makes that a minister with competencies unrelated to fiscal or social affairs can nevertheless weigh on decisions related to these topics affecting its constituency. Simultaneously, it is well known that most Belgian federal politicians do not spend their entire working week in the capital city where the federal ministries and the federal parliament are located. They divide their time between the capital city and their electoral district, to keep in touch with their constituents, even if they have not been simultaneously elected to a local office. Some have regular “visiting hours” at their municipality of residence, where they regularly receive questions and requests from their constituents.

The combination of this local presence, the bargaining power at the council of ministers, and the complicatedness and non-automatic application of important parts of the income tax and social security system suggest that the Belgian income tax and social security systems may be less formula-based than that they appear at first sight. There is scope for discretion by politicians to shape legislation, influence the implementation, or affect decisions in specific cases. The topic of the remainder of this paper is to answer the question on whether this influence indeed exists and to quantify it. Section 2 gives an overview of the distributive politics literature in the context of which our paper is to be situated. Section 3 describes the main dependent and independent variables we consider in our analysis. Section 4 consists of the actual empirical analysis of social and fiscal transfers and the quantification of the effect of ministerial representation. Section 5 draws a number of conclusions.

2 Literature overview

Our paper does not consider the political economy of income redistribution as such, but only in as far as this redistribution has a *geographical* impact. Its subject therefore is distributive politics as defined by Weingast et al. (1981 p. 644). The key starting point of the distributive politics literature is that politicians are elected locally to hold office nationally, and that political parties have to win votes locally -by means of income redistribution- to come to power nationally.

Three strands can be distinguished within the distributive politics literature. The first strand centres around the "common pool problem" and the "law of many districts", again with reference to Weingast et al. (1981). The second strand can be labelled the "core district" literature, with reference to Cox (2006). The third strand can be labelled the "swing district" literature, with reference to Lindbeck and Weibull (1987).

A first statement of the "common pool problem" literature is that the local election of politicians gives them an incentive to favour their geographical constituency with expenditure projects, including by means of income redistribution. This pays off when the bulk of the benefits of expenditure accrue locally, while the financing is borne nationally, through national taxation. A related hypothesis by Weingast (1981 p. 654) is that the lower the fraction of total taxes that is paid by a particular district, the higher its demand for central government expenditures will be, and hence the higher overspending will be. With more (and therefore smaller) districts, the tax price charged to a particular district for expenditures received will be lower, and hence the district's demand for expenditures will be higher. I.a. Elvik (1995) and Helland and Sørensen (2009) find empirical evidence for this "law of many districts" for Norway, while i.a. Hird (1991), DelRossi and Inman (1999) and Aidt and Shvets (2012) do so for the US.

A second prediction of the "common pool problem" literature is that overspending would increase with the number of politicians –ministers or members of parliament- representing an electoral district. We would call this second prediction the "law of well-represented districts" – as opposed to the "law of many districts". Examples of its empirical confirmation with respect to the number of parliamentary seats per district are Ansolabehere et al. (2002) for the US, and Jutras and Vaillancourt (2008) for Canada.

To a large extent the contribution of Weingast et al. (1981) disregards the important role that political parties play with respect to distributive politics in many countries. A second and third strand in the distributive politics literature fill this gap, respectively with the "core district" and "swing district" hypotheses.

Cox and McCubbins (1986) define *core voters* as voters with a strong preference for a particular party. The geographic version of the "core" hypothesis (Cox 2006) states that (risk-averse) political parties have an incentive to redistribute income in favour of districts where they obtain a majority of the votes (*core districts*) rather than those where no party has a clear majority (*swing districts*). In doing so, they prevent abstention of core district voters (labelled "mobilization" by Cox, 2006, p. 19) and

prevent emergence of competing political parties in their core districts (labelled "coordination" by Cox, 2006, p. 19). Otherwise stated: they prevent that core districts become swing districts in the next election.

Hence Cox (2006) predicts that the more a particular electoral district supports the governing coalition, the more it will be favoured with income redistribution. Cox' theory can hence be considered as the inclusion of the role of political parties into the "law of well-represented districts". Examples of empirical confirmation of Cox' "core district" theory are Levitt and Snyder (1995) and Bickers and Stein (2000) for the US Congress, and Ansolabehere and Snyder (2006) for US state governments.

Juxtaposed to the "core voter" hypothesis stands the "swing voter" theory of Lindbeck and Weibull (1987) and Dixit and Londregan (1995), which can be considered a third strand in the distributive politics literature. An example of empirical confirmation of the corresponding swing *district* hypothesis is Bickers and Stein (1996) for the US Congress[7].

Crampton (2004) and Milligan and Smart (2005) for Canada, and Helland and Sørensen (2009) for Norway, find evidence *both* in favour of the "law of well-represented districts" *and* in favour of the swing district hypothesis.

A final contribution to the distributive politics theory we consider is McGillivray (2004), as summarized in Golden and Picci (2008 p. 271-273). McGillivray integrates two extra variables into the "core district" / "swing district" hypotheses: (1) type of electoral system and (2) strength of party system.

Firstly, a proportional electoral system means that targeting swing districts is less rewarding for the ruling coalition compared to within a majoritarian system: the probability that being the largest party in swing districts would be decisive for holding office is (much) smaller in proportional electoral systems. Hence the ruling coalition is expected to target core districts, to prevent the emergence of new parties ("coordination" in Cox' terminology). The reverse is expected to hold in majoritarian systems, where emergence of new parties seems less of a threat to the governing party than in proportional systems.

Secondly, within a strong party system, there seems less room for individual powerful party members such as ministers to target fiscal transfers to their electoral district, compared to within a weak party system. This is because strong parties would make sure transfers are spent in the overall interest of the party rather than in the interest of powerful party members (McGillivray 2004 and Golden and Picci 2008 p. 271-272). Cox (2006 p. 17-19) argues, however, that the direction of extra transfers towards districts of powerful party members should not be interpreted as a sign of a weak party system. Crampton (2004 p. 16) gives the example of Canada,

[7] Dahlberg and Johansson (2002) and Johansson (2003) are sometimes cited as examples of confirmation of the "swing *district*" hypothesis for Sweden. However, as Cox (2006 p. 10) correctly points out, the Swedish studies are confirmations of the "swing *voter*" hypothesis of Lindbeck and Weibull, as opposed to the "swing district" hypothesis. The object of study of Dahlberg and Johansson are swing *voters* in different municipalities lying in several electoral districts, rather than the electoral districts themselves. They shed light on which municipalities obtain more discretionary grants from the central government *within* districts, not on the distribution of such grants *across* electoral districts.

with a strong party system, where "(…) all the stops are pulled to defend contestable seats held by Cabinet Ministers". Perhaps strong party systems allow extra transfers to ministers' constituencies to occur as a compensation of services provided by ministers to their party while holding office.

As Belgium is a country with a proportional electoral system, this suggests that we might expect incumbent political parties to steer redistributive transfers towards their core districts, all the more so because Belgium also has very strong political parties. It however seems unclear a priori to which extent such transfers to core districts could go together with government ministers favouring their own electoral districts with extra transfers. As political parties and the members of the executive taken together seem to be the key actors within the Belgian political system, we will test hereafter the influence of ministers on the geographical allocation of transfers in Belgium while controlling for the influence of political parties.

3 Description of the main dependent and independent variables

This study considers only cash transfers from and to households through social security contributions and social expenditures and personal income taxation. Social security benefits in kind, i.e. health care expenditures, are not included, due to lack of data. All data on transfers was obtained from the NBB website[8] for the period 1995-2010 at the administrative "arrondissement" level of geographical disaggregation.

The transfers considered represent a large share of the geographical fiscal transfers in Belgium and their budgetary impact is large. At the contribution side it comprises two of the three by far most important sources of funding of the federal government, i.e. the personal income tax and social security contributions. In 2010, Belgian personal income tax revenue and social security contributions equalled 28.2% resp. 32.5% of total federal tax revenue[9] (well above the resp. OECD averages). Total tax revenue in Belgium equalled 43.8% of GDP in 2010, the 3rd highest share of all OECD-countries. As mentioned before, at the benefits side, our dependent variable comprises all major social transfer categories in Belgium. According to the OECD, social spending in Belgium equalled 29.6% of GDP in 2010, again the 3rd highest share of all OECD-countries. When transposing these numbers to the household level, over our sample period household gross annual income appears to have been reduced on average with 18.2% by the income tax, while having been reduced on average with 27.1% by social security contributions, and having been increased on average with 26.0% by social expenditures payable to households.

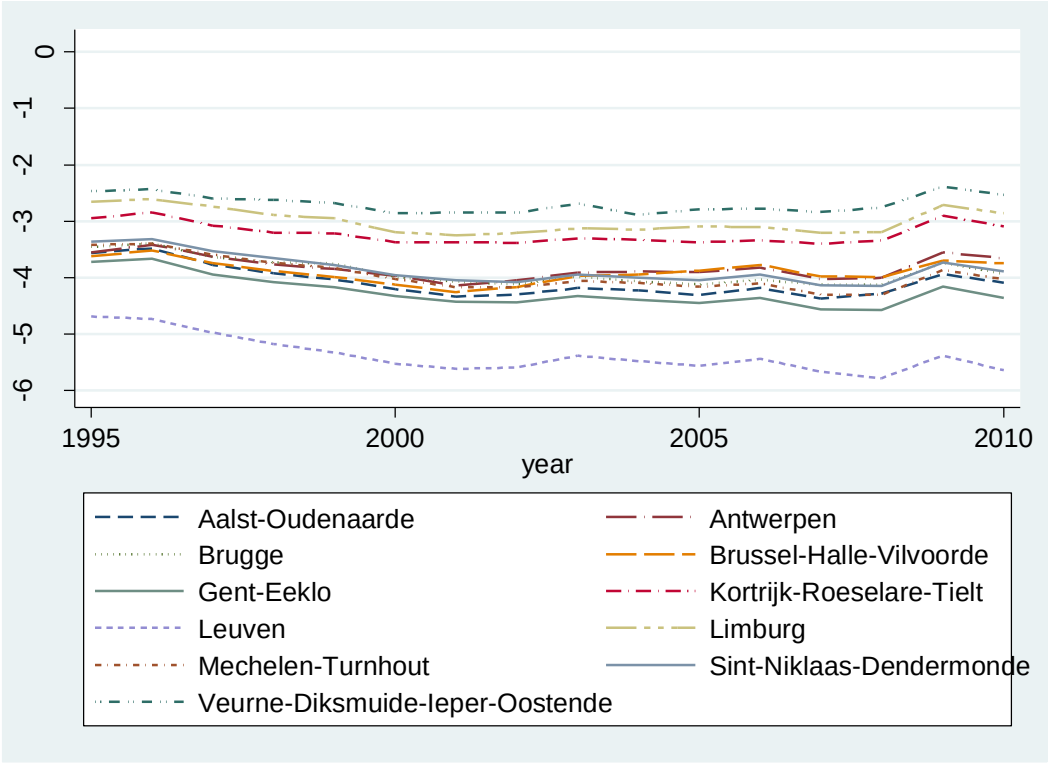
Graphs 1a (for the 10 Flemish electoral districts and the mixed Brussel-Halle-Vilvoorde district) and 1b (for the 9 Francophone electoral districts) show the evolution of cash transfers through social security contributions and expenditures and personal income taxes over time per electoral district, expressed in 1000 euros per capita terms, and deflated by the consumer price index for Belgium[10]. It is clear from the graphs that all electoral districts are net payers to the federal state. This is mainly due to the fact that our fiscal transfer data comprise the personal income tax and social security contributions at the revenue side, and only (part of) social benefits at the expenditure side.

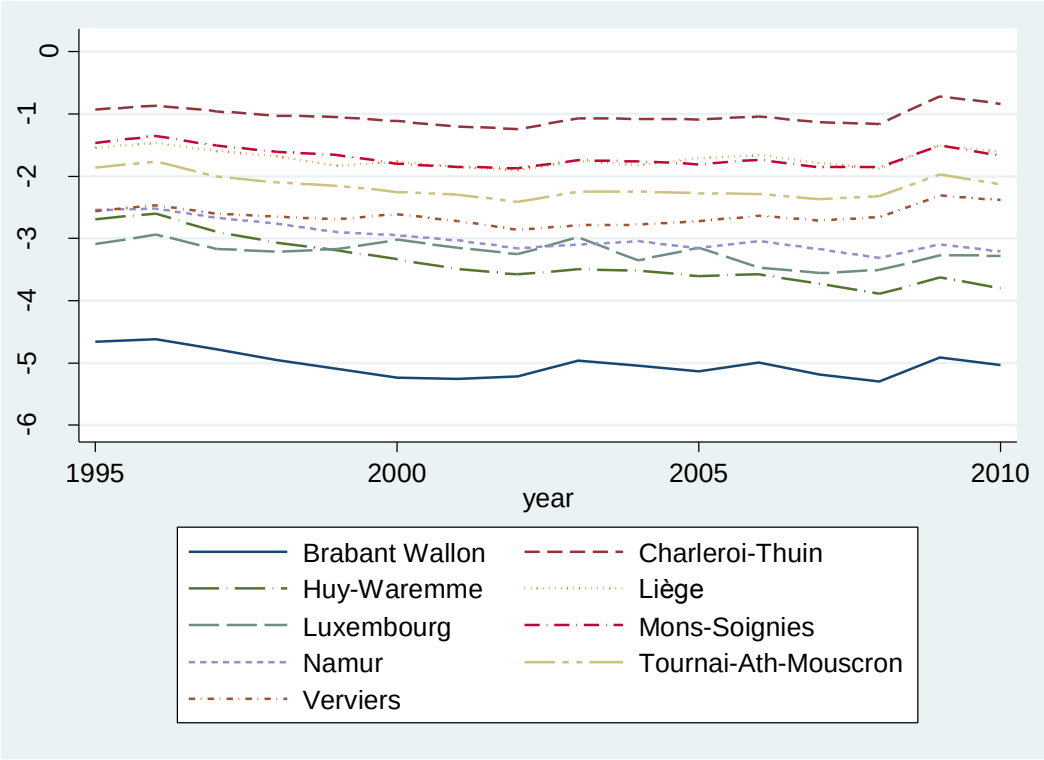
[8] www.nbb.be/doc/dq/n/dq3/NNR.pdf; this is the only source of geographically disaggregated Belgian federal revenue and expenditure data we are aware of.

[9] www.oecd.org; OECD statistics consider social security contributions as a part of tax revenue, which makes sense for Belgium, as social security contributions are mandatory, and as there is no one-on-one relationship between contributions and benefits any individual pays and receives.

[10] Taken from Statistics Belgium (http://statbel.fgov.be/).

Graphs 1a and 1b: The evolution of fiscal transfers per electoral district, in real 1000 euro per capita terms, for Flanders and Francophone Belgium resp. (1995-2010)





Source: NBB

The graphs also show large differences in net transfers between the electoral district paying the most per capita on a net basis and the electoral district paying the least. But perhaps most striking is the overall persistence of the relative positions of the electoral districts over time, with a number of Francophone districts persistently contributing the least of all districts to the federal government, and a number of Flemish districts persistently contributing among the most of all districts to the federal government. These quite large and persistent differences are obviously related to the equally large and persistent income differences between the typical Francophone Belgian electoral district and the typical Flemish electoral district. The persistence in large between-district-differences in net transfers will pose a challenge to our attempt in the analysis below to explain the relatively small within-district-changes in fiscal and social transfers over time by political factors. Nevertheless, some electoral districts did fare better or worse than others over time, with some changes in relative positions as a consequence.

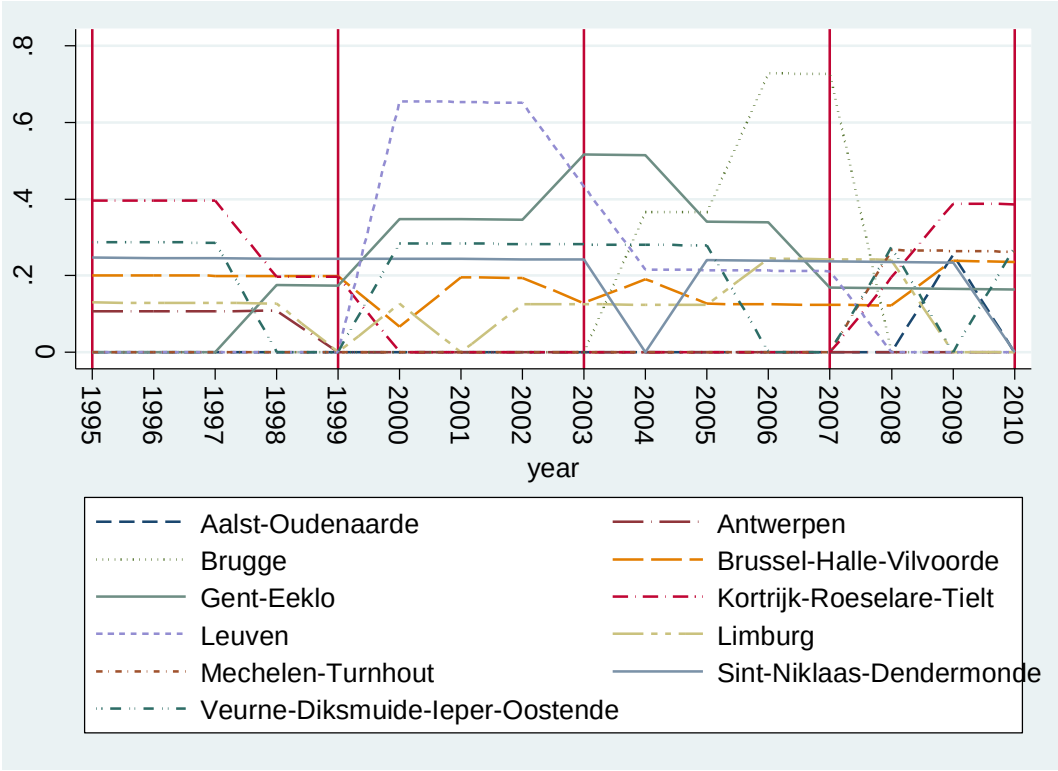
As for our major political variable, graphs 2a and 2b show the evolution of the number of federal ministers per 100.000 inhabitants for the separate electoral districts. Data was obtained from the federal parliament of Belgium and Wikipedia. For election years the distribution of ministers of the outgoing government was used. In these graphs, for the years after 2002, when the "provincial" districts reform was voted into law, we kept on assigning ministers to the old electoral district they had run for in the previous federal election. Newly elected ministers were assigned to an old electoral district on the basis of residence.

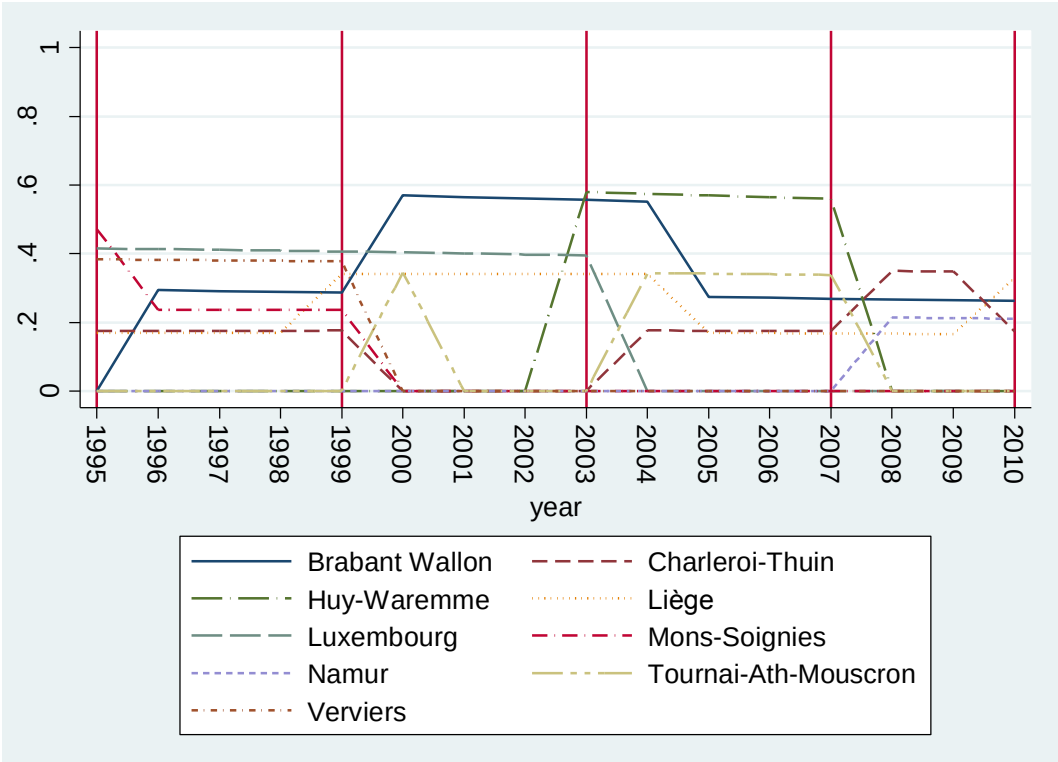
Graphs 2a and 2b reveal not only considerable variation between electoral districts in terms of the number of ministers elected in a particular district per 100,000 inhabitants, but also –in contrast to the transfers shown in graphs 1a and 1b- considerable variation over time[11]. In 62 of our 320 district-year observations, the absolute number of ministers per district changes. The graphs show that –unsurprisingly- changes in (provenance of) ministers occur mainly in the years following federal election years (i.e. in years 1996[12], 2000, 2004, and 2008). The red vertical bars in the graphs mark election years.

[11] So called secretaries of state were not counted. Secretaries of state are in charge of policy areas considered less important and they are not allowed to participate in the weekly meetings of the "Council of Ministers", i.e. the entire cabinet of ministers, which jointly takes the main government decisions. Their only way to influence the decisions of the Council of Ministers is by requesting the Minister to whom they are "attached" to make their case. Also, the budget at the disposal of secretaries of state to hire personal advisors so as to staff their private "cabinets" appears considerably smaller than that of federal ministers. As a robustness check, we have included these junior ministers into our regression analysis below. The association between ministers –including secretaries of state- and transfers remains highly significant, but the size of the coefficient becomes somewhat smaller. This is consistent with the findings of Golden and Picci (2008 p. 286) for Italy.

[12] As the result of the 1995 elections was the continuation of the outgoing federal government coalition, minister changes were very limited in 1996.

Graphs 2a and 2b: The evolution of the number of ministers per electoral district, per 100,000 inhabitants (1995-2010)





Sources: Wikipedia, federal parliament of Belgium, own calculations

But there also seem to be a lot of exceptions to this rule, i.e. 33 out of 62 minister changes did not occur in post-election years. This is because, as opposed to members of parliament, politicians are not elected to be a minister (nor re-elected) for an entire legislature, but appointed (and dismissed) for a non-predetermined time period. Formally appointments are made by the head of state, but de facto the positions are divided between parties after the elections during negotiations between party presidents. When deemed necessary, party presidents appoint another person between elections[13]. E.g. the Leterme – Van Rompuy governments between 2008 and 2010 consisted of several ministers who had not participated or had not been elected in the preceding 2007 federal elections. However, most of the time, such unelected ministers participate in subsequent elections (and mostly successfully). The large number of changes in political representation which happen between elections inspired us to consider a subset of them as exogenous to fiscal transfers and use them as instruments to identify a causal effect of political representation on transfers in section 4.

Finally Table 1 presents summary statistics for our socio-economic control variables for the year 2010. The reported standard deviation therefore only reflects differences between districts. The numbers show that the significant differences between districts in terms of transfers per capita which were shown in Figures 2a and 2b also hold for socio-economic variables such as the share of unemployed in the total population.

Table 1: Means and between standard deviations for socio-economic variables per electoral district (2010)

	Transfers (1000 euros per capita)	**Gross income (1000 euros per capita)**	**Minors (% of population)**	**Unemployed (% of population)**	**Retired (% of population)**
Mean	-3.29	17.14	22.94	3.93	15.06
Standard deviation	1.19	2.42	1.59	1.70	1.77

4 Empirical analysis

Weingast's politico-economic mechanism described in section 2 can be summarized as a system in which -in our case- ministers cater to their constituency in order to gain votes, and in which the electorate in turn rewards ministers for their services. We represent this in a stylized way through the following system of equations (omitting other covariates, as well as indices for electoral districts or time):

$$T = b_1 M + u \qquad (1)$$
$$M = b_2 T + e \qquad (2)$$

The first equation of this system states that transfers *T* to some electoral district are a linear function of a measure of ministerial (over)representation *M.* The second equation models ministerial (over)representation *M* of an electoral district as a linear function of transfers towards an electoral district.

Solving this system of equations for the equilibrium amount of ministerial representation shows

$$M = \frac{b_2 u + e}{1 - b_1 b_2} \qquad (3)$$

If we would estimate equation (1) in isolation, ignoring the simultaneous determination of the variables described by the full system of equations, the OLS estimate of coefficient *b1* is given by

$$\beta_1 = b_1 + \frac{cov(M,u)}{var(M)}$$

Using the solution for *M* from equation (3) and its dependence on the error term *u*, this can be written as

$$\beta_1 = b_1 + \frac{b_2}{1 - b_1 b_2} \frac{var(u)}{var(M)} + \frac{1}{1 - b_1 b_2} \frac{cov(e,u)}{var(M)} \qquad (4)$$

Expression (4) shows that the OLS estimate of equation (1) in isolation is in general biased. The second term shows the effect of ignoring reverse causality, in case transfers affect ministerial representation (for example by increasing re-election chances) such that $b_2 \neq 0$. The third term shows the effect of the presence of common or correlated omitted variables in both equations causing $cov(e,u) \neq 0$.

In our empirical application, we will attempt to limit omitted variable bias by including dummies per electoral district to capture any time-invariant differences between electoral districts, as well as year-dummies capturing shocks common to all districts in a certain year, and district-specific year-trends to accommodate any omitted trending variables. In addition, we include the average district income and

the share of dependent inhabitants (retired, unemployed, minors) as economic and demographic controls. These variables clearly affect a district's transfers, but omitting them would only bias estimates should these variables also affect the average ministerial representation of a district – and it is not straightforward to imagine how this would work. In one specification we control for other political variables which could affect a district's transfers such as the share of parliamentary seats held by the governing coalition in that district, and could also affect the ministerial representation of that district (say if the coalition rewards that district with a minister if it delivered a lot of parliamentary seats to the coalition). Given the inclusion of these controls, we might hope to minimize the omitted variable bias.

In section 0, we will ignore the simultaneity bias and estimate equation (1) in isolation, while adding controls as described above. As shown by expression (4), when omitted variable bias is properly controlled for, the estimated coefficient on the ministerial representation variable *M* will be a combination of the true coefficient *b1*, and the coefficient determining the reverse causality *b2*. The estimation results in this section therefore do not have a causal interpretation, as they do not reflect the effect of an exogenous change in the ministerial representation of a district on the incoming transfers. Nevertheless, we find it important to report these results, as finding significant correlation between transfers and ministerial representation implies that *either* $b1 \neq 0$ *or* $b2 \neq 0$ *or both*. As such, a significant coefficient in the single equation framework is interesting in itself, as it suggests some political-economy mechanism may be at work causing the observed correlation between ministerial representation and transfers.

In section 0, we will identify two separate sets of exogenous changes in political representation which we use to identify the causal effect of political representation on transfers by means of an instrumental variables analysis. Using a subset of exogenous changes in ministerial representation, M*, which are unrelated to current and past transfers, we are able to obtain unbiased estimates as by assumption cov(M*,u)=0.

4.1 Estimation while ignoring the endogeneity of political representation

Table 2 shows the results of estimating equation (1) while ignoring any simultaneity bias. All specifications include a full set of district-level dummies, which equates to a "fixed effects within estimation" considering only variation in the data within electoral districts by subtracting the district-specific means for each variable. This removes any bias caused by time-constant factors (for example those related to political and economic history) within electoral districts which are correlated with both the amount of transfers and our major political variable of interest, i.e. ministers per district. Moreover, we add year dummies to control for any effects common to all districts in a specific year, and district-specific year trends to control for any omitted trending variable which might be specific to a district[14]. All reported standard errors are grouped (clustered) on the district level, allowing for heteroskedasticity and arbitrary autocorrelation between observations within each district.

The specification reported in column (1) of Table 2 uses OLS to estimate the association between *transfers* and the contemporary value and up to three lags of *ministers*. The coefficients on the subsequent lags of *ministers* suggest that the correlation between ministers and transfers extends and grows over time. Adding up the coefficients for all lags, we find that after four years, an extra minister in a district goes with an estimated transfer increase of 21 euros per capita per year[15]. The 95 percent confidence interval of this amount is [11,31]. The average number of inhabitants of an electoral district over our sample period is 519,086. This means that an extra minister is associated on average with an extra 10.846 million euros per year for his district.

Column (1) additionally controls for the average *gross income* per district (in logs), as well as for the share of dependent individuals (retirees, unemployed and minors) in the total population. As expected, a higher average gross income goes with lower transfers to a district, and a higher share of dependent inhabitants is associated with more transfers. The gross income variable is highly correlated with many key socio-economic variables, making it difficult to separately estimate the relation between individual key socio-economic variables and transfers. The share of dependants has a correlation coefficient of -0.85 with gross income, and in some specifications which we will discuss below both variables will not be jointly significant. The inclusion of 20 district dummies and 20 district-level year trends also implies that a lot of variation within districts is already accounted for in all specifications, making it hard to additionally estimate the effect of slowly changing variables such as income or the share of dependants (unlike the more erratically changing political representation). Nevertheless, measures of fit suggest that these year-trends should be included, so we opt to include them to control maximally for omitted variable bias, at the cost of a loss of precision in the point estimates of other covariates.

[14] The estimation results for these dummies and district-level year trends are omitted from all tables.

[15] It should be kept in mind in these calculations that the dependent variable *transfers* is measured in 1000 euro per capita, and the *minister* variable is measured in ministers per 100,000 inhabitants.

Table 2: Estimating the association between ministers and transfers

	(1)	(2)	(3)	(4)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	OLS	OLS	sys-GMM	sys-GMM
$transfers_{t-1}$		0.158*	0.894***	0.941***
		(0.0899)	(0.0795)	(0.0628)
$logpriminc_t$	-4.387***	-4.138***	-1.077**	-0.857*
	(0.384)	(0.449)	(0.444)	(0.415)
$dependants_t$	5.849**	5.682**	0.793	0.468
	(2.708)	(2.198)	(1.094)	(0.877)
$ministers_t$	0.0152			
	(0.0296)			
$ministers_{t-1}$	0.0262	0.0725***	0.0711***	0.0766***
	(0.0305)	(0.0205)	(0.0247)	(0.0246)
$ministers_{t-2}$	0.0666			
	(0.0445)			
$ministers_{t-3}$	0.101***			
	(0.0272)			
$govtseats_t$				-0.0711
				(0.0508)
$seatsmargin_t$				0.0171
				(0.0219)
N	260	300	300	300
Instruments	-	-	41	43
AR(1) p-value	-	-	0.08	0.08
AR(2) p-value	-	-	0.398	0.371
Hansen p-value	-	-	0.488	1

District dummies, year dummies, and district-year trends included in all specifications. Robust standard errors clustered at the district level in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

As the association between ministers and transfers appears to increase over time, column (2) considers an alternative dynamic specification with a lag of the dependent variable included as an explanatory variable. A single one-year lag of the *ministers* variable is used, as it is likely that ministers require some time in office before they may start to affect decision-making and transfers. This specification is quite succinct, but it allows to differentiate between the short and long run association between political representation and transfers, and conforms to our intuition that the amount of transfers towards a district might be slow to adjust and may be conditional on past levels even when taking into account the different covariates in our model. However, it is well known that the coefficient on the lagged

dependent variable is biased downward when estimating using OLS in the presence of fixed effects, and indeed the coefficient on lagged transfers seems small.[16] This bias might also affect the other coefficient estimates. The simple OLS results are nevertheless interesting, as the instrumental variable methods designed to address the possible bias due to a lagged dependent variable might also be subject to possible biases (see Roodman 2009a). Moreover, the bias of the OLS specification might be limited in our application since the number of time periods is relatively large with T = 16.

The specification shown in column (3) takes first differences of the equation in levels to remove the fixed effects, and uses a GMM instrumentation approach to control for the endogeneity of the transformed lagged dependent variable (but does not yet control for the endogeneity of our main variable of interest *ministers*). Following Arellano and Bond (1991), the second to fourth lag of the levels of the lagged dependent variable are used as instruments for its lagged differences (difference GMM). Simultaneously, differences of the lagged dependent variable are used as instruments for the equation in levels (system GMM) as in Arellano and Bover (1995) and Blundell and Bond (1998). All estimation was performed using the `xtabond2` command in Stata (see Roodman 2009b, for details). The results indeed suggest that the coefficient on the lagged dependent variable was severely downward biased in the OLS estimation. The reported standard tests for autocorrelation allow to reject the presence of first order autocorrelation in the residuals, but not of second order autocorrelation, which matches the assumptions of this estimator. The Hansen over-identification test does not allow to reject the validity of the over-identifying assumptions, but given the large number of instruments, the power of this test is probably low. A robustness-check in section 4.2.3 shows that in specifications with a reduced instrument set, the main conclusions of our analysis remain to hold and the validity of the over-identifying restrictions is upheld.

In the specifications with a lagged dependent variable and a single lag of *ministers*, the estimated association between the one year lag of *ministers* and transfers in year *t* is given by $b \sum_t a^{t-1}$, with *b* the estimated effect of the one-year lag of *ministers,* and *a* the coefficient on the lagged dependent variable. For large *t*, this effect converges to $b/(1-a)$. As it turns out, this estimated long run effect is not significantly different from 0 for the estimates reported in column (3), due to the precision of the estimates, but especially due to the fact that *a* is close to 1. However, in the perhaps more relevant `medium run', of –for example- 8 years, or 2 full terms for a minister, the estimated association is about 40(20) euro per capita, is significant different from 0 on the 10-percent level, and is both statistically and economically significantly larger than the effect of 7.9(2.5) euro after the first year. The estimate after four years equals 24(10) euro, which corresponds rather closely to the estimate of 21(5) euro after four years from the OLS estimation in column (1).

While the specification behind columns (1) to (3) may be considered as a test of the "law of well-represented districts" applied to government ministers, column (4) presents the results when simultaneously testing the core district and swing district

[16] Reversely, estimating this specification using pooled OLS, excluding all district dummies and year trends results in an estimated coefficient of 0.96(0.02), which is upward biased, and therefore can be seen as an upward bound to the true value.

hypotheses. The specification adds the share of parliamentary seats held by all governing parties[17] combined per district *govtseats* (similar to Golden and Picci (2008)) to test the "core" hypothesis, with an expected positive association. To test the "swing" hypothesis, we estimate the relationship between net transfers and (the absolute value of) the difference between the seats share of the largest party per district and the second largest party *seatsmargin* (similar to Crampton (2004) and Milligan and Smart (2005)). The expected sign of this variable is negative. Column (4) shows that the estimated coefficients of both variables have both the unexpected sign but that they are not significant. Also note that adding these controls does not materially change the coefficient of the *ministers* variable, nor its significance[18].

The *seats* variables could also serve to control for some form of endogeneity in the *minister* variable: it could well be the case that the governing parties award ministerial posts to (party members of) their core districts, as a reward for the many seats won in the preceding election. Alternatively, the governing coalition could well award a minister to a district particularly if it is a “swing” district –e.g. if the governing coalition only won a small majority of the parliamentary seats during the preceding election in that district. With this latter strategy the governing coalition could envisage bringing extra visibility in view of the next election. Hence, the geographical distribution of ministers could be a tactical instrument. In both cases, it would be the share of parliamentary seats held by the governing parties (and not the choice of the party president) that would drive the number of ministers in a particular electoral district. However, our results suggest that such mechanisms only play a small role or cancel out, as the correlation between *ministers* and the two *seats* variables is close to zero over our sample period, and as controlling for core and swing variables does not materially affect the estimated effect of *ministers*[19].

Next, we test if the effect of *ministers* is different for the two major components of income redistribution in Belgium which make up our dependent variable *transfers*, i.e. the social security system and the personal income tax. The effect of political representation might differ between these two pillars of income redistribution, as they belong to the competencies of different federal ministers and ministries. Moreover, trade unions and employers’ federations have an important formal say in social security policy in Belgium[20], but not in income tax policy.

Table 3 shows the results of repeating the regression after decomposing our dependent variable, while using the system-GMM technique as in columns (3) and (4) of table 2. Overall, the fit of the specification seems worse than before, with

[17] Over our entire sample period, all federal governments were coalition governments. From the year 2004 onwards, for any of the 20 old electoral districts the seats distribution of the “provincial” electoral district it is part of, has been used.

[18] Also when operationalizing the *govtseats* and *seatsmargin* variables in different ways, the estimated coefficients are mostly insignificant. Most importantly they do not affect the size nor the significance of our major variable of interest, *ministers.*

[19] Alternatively, it may be concluded from column (3) that for an electoral district, an increase in members of parliament supporting the governing coalition is not associated with an increase in transfers, as opposed to the findings of Evans (2005) and Jutras and Vaillancourt (2008) for Canada.

[20] Some observers claim even a bigger say than the resp. ministers in charge, and complaints are heard from members of parliament that when they vote the budget, they have largely been kept out of the budget preparation for social security by the trade unions and the employers federations, despite the magnitude of the social security budget.

primary income not significant in explaining social security transfers (which seems highly unlikely), and a rejection of the absence of AR(2) in the residuals. The size of the estimated association between *ministers* and *social security* related transfers is larger when compared to the estimate for *income tax* payments. The latter point estimate is insignificant at the 10% level, however. As we cannot reject that the effects are equal for both types of transfers, we will continue to group them together and study the effect of political representation on the total transfers.

Table 3: Separate analysis for our two transfer categories

	(1)	(2)
Dependent variable:	transfers through *income tax*	transfers through *social security*
Estimation method:	sys-GMM	sys-GMM
$income\ tax_{t-1}$	0.746***	
	(0.112)	
$social\ security_{t-1}$		1.040***
		(0.0912)
$logpriminc_t$	-1.030**	-0.149
	(0.438)	(0.251)
$dependants_t$	0.838	-0.186
	(0.918)	(0.487)
$ministers_{t-1}$	0.0136	0.0396**
	(0.0134)	(0.0145)
N	300	300
Instruments	41	41
AB-AR(1) p-value	0.072	0.044
AB-AR(2) p-value	0.754	0.073
Hansen p-value	0.989	1

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Instrumenting changes in political representation

Previous contributions (such as Crampton, 2004, for example), do not consider the possible endogeneity of political variables when explaining geographical redistribution. Milligan and Smart (2005, p. 14) claim that, because in a strong party system like Canada's ministers are appointed by their prime minister, they are exogenous to transfers. A similar reasoning could be applied to a strong party system like Belgium's, where party presidents of the governing coalition appoint ministers. However, one might still argue that *ministers* are endogenous due to reverse causality. It is likely that, even in a strong party system, when a candidate obtains many personal votes during the preceding election, his party president will find it harder to deny this candidate a ministerial post. Obtaining many votes could in turn be due to having channelled extra transfers to one's district. If this is the case, *ministers* would be driving *transfers*, but simultaneously *transfers* in a district would be driving *ministers*. As a matter of fact, it is hard to imagine a mechanism where ministers would channel transfers towards certain districts which cannot be explained by purely socio-economic factors, if not for attracting votes and either directly or indirectly affecting the probability of re-election or re-appointment. To isolate the causal effect of ministerial representation on transfers toward a district, we therefore will not assume that ministers are appointed exogenously.

As before we will use difference and system GMM estimation to address the bias introduced by the presence of a lagged dependent variable; but we will now simultaneously instrument using changes in *ministers* which may be reasonably thought to be independent from transfers. We develop two alternatives to isolate such exogenous minister changes. A first approach is to exploit the resignation of *ministers* following various events (two examples are ministers resigning after the escape of a top-criminal and a food scare), policy disagreements within the ruling coalition (such as a controversial arms deal and an intra-coalition row over airplane noise over Brussels), or appointments of ministers in international institutions. In a second and separate IV analysis, we exploit the changes in political representation which occurred due to redrawing of electoral district borders in the reform of 2002.

As a robustness-check we perform a third IV analysis where we use the system and difference GMM approach not only for the lagged dependent variable, but also use this approach for the political representation variable, instrumenting differenced changes in ministers with the lagged level, and vice-versa. This approach is similar to Golden and Picci (2008 p. 286).

4.2.1 Instrumenting using exogenous changes in political representation[21]

Table 5 in Annex 1 gives a detailed description of 12 decreases in *ministers* over our sample period (out of a total of number of 62 changes in *ministers*), which we argue to be exogenous to the transfers received in a region.[22] These 12 cases are due to ministers resigning (or being forced out of) their posts. Most of these 12 resignations correspond to another change in ministerial representation as in most cases the resigning ministers are replaced. Our results do not change much if we include these corresponding positive changes in the instrument set. However, at the next elections, these newly appointed ministers may be more likely to be re-elected if they channeled transfers to their constituency, and their presence then would become endogenous to the transfer game. We therefore excluded these corresponding positive changes from our analysis.

None of the 12 considered resignations are related to the preceding federal elections, nor to sub-central or European elections. They mostly comprise hard to foresee cases where ministers have been forced to resign because of some scandal for which they took the political responsibility, or chose to step down due to some disagreement within the governing coalition. All 12 are clearly unrelated to the "fiscal transfer game"[23].

Table 4 reports the results of our IV regressions. As before, all specifications use difference and system GMM, with the second to fourth lag of the lagged dependent variable as instruments in the differenced equation, and first differences values of the lagged dependent variable as instruments in the level equation.

The specification in column (1) uses the subset of decreases in *ministers* which are due to a limited set of assumed exogenous causes (reported in table 5) as an instrument for the original *ministers* variable. This subset of changes is only used as an instrument for the equation in first differences in the GMM system. The resulting estimated causal effect of *ministers* on *transfers* in column (1) corresponds to a short run effect of 20 euro. Despite retaining only 12 out of 62 changes in ministers as exogenous to the transfer game, this estimate is both large and highly significant from 0. The estimated long run effect using this specification is large 214(244) euro, but is imprecisely estimated. The effect after a single term of four years is 75(34) euro, which is significantly larger than 0, and significantly larger than the short run effect. These effects are also large when compared to the results obtained in Table 1, ignoring the possible endogeneity of *ministers.*

[21] We are grateful to Liesbeth Colen for her suggestions regarding the implementation of this IV strategy.

[22] Using a mere subset of the original and possibly endogenous independent variable as the IV is an IV strategy similar to the one implemented in Dahlberg et al. (2008) when investigating the flypaper effect for Swedish municipalities. They use a *census-determined* (i.e. partial) change in grants to instrument the *actual* (i.e. total) change in grants received by Swedish municipalities.

[23] A resignation unrelated to the "fiscal transfer game" which we did not include in table 5 is the appointment of then prime minister Van Rompuy to the post of President of the EU Council of Ministers in 2009. Because of the government reshuffle that followed Van Rompuy's resignation, Van Rompuy's electoral district, Brussel-Halle-Vilvoorde not only lost a minister, but also gained a minister (Vanackere). The net effect of Van Rompuy's resignation on his district's number of ministers hence was zero. Our estimation results are robust to including Van Rompuy's resignation into our set of exogenous minister changes.

Table 4: Effect of ministers on transfers, IV estimation.

	(1)	(2)	(3)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	sys-GMM	sys-GMM	sys-GMM
$transfers_{t-1}$	0.899***	0.864***	0.874***
	(0.0901)	(0.108)	(0.0789)
$logpriminc_t$	-1.267**	-1.609***	-1.256**
	(0.502)	(0.555)	(0.484)
$dependants_t$	0.684	0.452	0.794
	(1.261)	(1.638)	(1.191)
$ministers_{t-1}$	0.200**		0.118***
	(0.0788)		(0.0388)
$newministers_{t-1}$		0.273***	
		(0.0855)	
N	300	240	300
Instruments	41	39	44
AB-AR(1) p-value	0.097	<0.00	0.081
AB-AR(2) p-value	0.446	0.074	0.41
Hansen p-value	1	1	1

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2.2 Ministers changes due to electoral districts merger as an instrument[24]

As a second IV strategy, we exploit changes in political representation caused by the major electoral district reform that took place over our sample period, i.e. in 2002 (see Introduction). As the maps in Annex 3 show, on the occasion of the 2002 reform the former 20 federal electoral districts were reduced to 11. To construct an instrument on the basis of this reform, we give up our earlier assumption that ministers continue to cater only for their original electoral district even after this district has been merged with one or more other electoral districts into a new, larger electoral district. We now re-calculate our measure of political representation to reflect the fact that some old districts obtained extra ministers when they were merged with an old district which had one or more ministers. We assume that ministers distribute their attention over the respective smaller old districts within their provincial constituency according to the smaller districts' population shares.[25] We denote this newly calculated variable by *newministers*.

We subsequently instrument the newly defined *newministers* variable using as an instrument only the subset of changes in *newministers* which are due to the merger, denoted *mergerministers*. This instrument is presumably exogenous, if we are willing to assume that voters in the elections of 2003 did not vote with forward looking transfer-related expectations regarding ministers they could additionally vote for due to the electoral district merger, and that ministers did not anticipate the merger and did not steer transfers before the merger to their future new constituency. To the extent that these assumptions fail to hold, our estimate would in part still reflect a reverse causal effect.

Table 6 in Annex 2 describes our instrument over the 2003-2007 period. We assume that our instrument takes values different from zero only over this period, assuming that ministers started at the earliest to steer transfers to their "new districts" in 2003. 2003 is the year of the first elections after the merger law was voted –in September 2002-, so we may assume that the "new" kind of transfers did not arrive in time to influence the election outcome of 2003[26]. We drop all observations after 2007 from the analysis, because 2007 was another federal election year after which ministers could have been appointed (or not) because they had been successful (or not) in steering transfers. Hence after 2007 we cannot safely assume anymore that our instrument is exogenous to transfers.

[24] We are grateful to Matz Dahlberg for suggesting this IV to us in the context of our analysis.

[25] For example, a smaller district with 100,000 inhabitants having one minister originating from that district, and which represents 1/3 of the total population of a newly instituted larger district, has a value of *ministers* (which is the number of ministers expressed per 100.000 inhabitants) of 1 when ignoring the reform as we did before, and a value of *ministers* of 1/3 when recalculating the variable to take into account the redrawing of constituencies as we do in the context of this IV strategy.

[26] Our regression results reported below are hardly affected if we assume –as a robustness check- that ministers were already appointed in 2002, to reflect that they may already had started steering transfers to "their new districts" from 2002 onwards, anticipating the electoral boundary reform. Effectively, this reform was extensively debated in parliament and in the press before it was voted. But it was also quite heavily disputed before it was submitted to parliament, even within the governing coalition. After it was voted in parliament, opposition parties continued opposing it, including in court.

It is clear from table 6 that only a limited number of old districts obtained ministers because of the 2002 district merger. These are the 12 old electoral districts belonging to the provinces of Hainaut, Liège, Oost-Vlaanderen and West-Vlaanderen. The boundaries of the other old electoral districts Brussel-Halle-Vilvoorde, Leuven, Limburg, Luxembourg and Namur were left unchanged. The old districts of the province of Antwerp were merged during the reform, but this does not affect our instrument as there was no federal minister originating from these districts during the 2003-2007 period. The last column of table 6 gives an overview of the values different from zero taken by our instrument *mergerministers*. The correlation between our *newministers* variable and our instrument *mergerministers* over the considered time period is quite high, with a correlation coefficient of 0.35.

Column (2) of Table 4 reports the results of the IV analysis using these changes due to the redrawing of electoral districts as an instrument. The coefficient of *ministers* in column (2) is 27 euros, which is significant at the 0.01 level. Oddly, despite the fact that apart from the shorter sample and the different instrument, the specification is similar to the specification in column (1), the AR(2) test now rejects the absence of an AR(2) process in the residuals, which would invalidate the analysis.

4.2.3 Robustness

As a robustness check, column (3) of Table 4 shows the results when instrumenting lagged differenced *ministers* not with an external instrument, but rather using only the past information contained in its own second to fourth lagged levels. The coefficient on *ministers* is significantly different from zero, and smaller than the estimated obtained when using the set of resignations or the border changes as an instruments.

As the Hansen IV-overidentification test loses power as the instrument set becomes larger, we re-estimated our main specifications using a restricted instrument set. More specifically, we drop the equation in levels, using only the equation in first differences (difference GMM). We drop *dependants* as an explanatory variable (as it is highly co-linear with income and quite often insignificant), we instrument the first-differenced lagged dependent variable only with the second and third lagged levels (rather than including the fourth), and do not include district-year trends but only the full set of year-dummies. This reduces the instrument count considerably in all specifications. The year-dummies account for 14 of the remaining 18 instruments in the specification in column (1). The results in Table 5 show that our main results are qualitatively the same despite these many changes in the specification, and that the Hansen tests again do not allow to reject the validity of the reduced instrument set.

Table 5: Effect of political representation as measured by IV estimation, with a restricted instrument set

	(1)	(2)	(3)
Dependent variable:	$transfers_t$	$transfers_t$	$transfers_t$
Estimation method:	diff-GMM	diff-GMM	diff-GMM
$transfers_{t-1}$	0.861***	0.826***	0.838***
	(0.114)	(0.137)	(0.127)
$logprimin c_t$	-1.436**	-1.700*	-1.495*
	(0.627)	(0.824)	(0.724)
$ministers_{t-1}$	0.152**		0.113**
	(0.0609)		(0.0417)
$newministers_{t-1}$		0.271***	
		(0.0891)	
N	300	240	300
Instruments	18	16	19
AB-AR(1) p-value	0.079	<0.000	0.067
AB-AR(2) p-value	0.419	0.061	0.393
Hansen p-value	0.273	0.919	0.221

District dummies, year dummies, and district-year trends included. Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

It is noticeable that all IV estimates are larger than the OLS estimates. This suggests a downward bias in OLS, rather than an upward bias, which is opposite to what we would expect from equation (5) with b1>0 and b2>0. Hence this runs counter to our intuition about the effect of transfers on political representation (through improving re-election chances). The effect might be purely econometrical, however, if the IV estimate removes some measurement error in the original independent variable. Another possibility is that there would be a difference in how some types of political representation have a stronger effect on transfers. One can imagine e.g. that ministers who suddenly need to compete for votes within a substantially enlarged electoral district (see maps in Annex 3), have a stronger incentive to channel transfers towards their constituencies, in order to secure visibility with the local electorate before the next elections. These effects may dominate the removal of the bias through reverse causality, which might be small even in the OLS analysis as the variation over time in transfers *var(u)* is small compared to the variation in *ministers var(M).*

5 Conclusion

Controlling for key socio-economic variables such as the local share of dependants in the population and average gross income, we find that per capita cash transfers to a Belgian electoral district are 21 euros per capita and year higher for every federal minister originating from that electoral district. This amounts on average to an extra 10.846 million euros per year per minister in a district. These estimates are of economic significance, especially when considering that these transfers may accrue to a subgroup of people within each district.

The observed correlation between ministerial representation and transfers might result from a mechanism where ministers cater for their constituency in the form of transfers and are in turn rewarded by voters. Although we believe this empirical result is interesting in its own respect, further research would be required to prove or disprove the existence of such a mechanism. We subsequently rather focussed on estimating the effect of exogenous changes in ministerial representation on transfers, using two IV strategies. Although the point estimates differ considerably between specifications and appear large when compared to the OLS estimates, the different IV analysis and robustness checks are broadly supportive for the hypothesis that ministers steer transfers to their constituency.

Our findings contribute to the literature on distributive politics in several ways.

Firstly, regarding the types of transfers we consider, virtually the entire empirical distributive politics literature appears to deal with “pork barrel”, not with -supposedly-formula-based fiscal transfers to citizens, which make up the core of income redistribution in most developed countries. Our setup and findings can be juxtaposed to the setup of Dixit and Londregan (1998 p. 512) who assume that income taxes would rather serve ideological purposes and targeted handouts would serve tactical purposes; or Milesi-Ferretti et al. (2002 p. 647-648) who assume that public purchases of goods and services are easier to target geographically compared to transfers which are easier to target across social groups. By providing quantitative evidence of political influence in income redistribution, we provide support to qualitative claims such as by Besley and Coate (2003 p. 2628) who state that “even when spending is allocated formulaically, it is possible for legislators to manipulate such formulas to favour their own districts”.

Secondly, regarding the type of political representation we consider, we do not know of other research explaining the geographical distribution of –supposedly-formula-based fiscal transfers between government and citizens through income taxation and social security by the geographical distribution of cabinet ministers. Persson and Tabellini (1999) find that proportional electoral systems –such as in Belgium- encourage “broad-based targeting”, as opposed to the “narrow targeted transfers” favoured in majoritarian systems, but the authors do not specify whether such targeting is implemented by the executive, by party presidents or by the legislative.

Finally, to our knowledge this paper is also the first to demonstrate for the case of Belgium that political factors matter with respect to fiscal transfers between the federal government and citizens.

With respect to role of strong political parties as analysed by the distributive politics literature, we do not find robust evidence that in Belgium the governing coalition is able or willing to steer fiscal transfers to core or swing districts. This could simply mean that the interests of ministers and their political parties overlap[27]. It could mean that Belgian political parties reward their ministers with extra transfers for their districts in return for services rendered by these ministers to their party while holding office. Alternatively, it could point at strategic behaviour of ministers towards their party, i.e. at a principal-agent problem between political parties (the principal) and ministers (the agents) going on in Belgium.

A number of questions remain unanswered. Although we argued that ministers might pressure their colleagues in the council of ministers, or within their party, as an explanation for our finding of a minister effect *across the board*, i.e. including for ministers not having social security nor income taxation under their remit, further research is required in this area, it would be interesting to consider whether some ministers have more influence compared others, or whether ministers affect transfers more within their own domain. Another question we left open is which channels ministers use to direct "taxing & spending" towards their electoral district. Do they design specific rules or rather bend their implementation? While we suggested possible mechanisms in the introduction of this paper, our regression analysis does not provide evidence in favour of nor against one or the other. However, a challenge for such a line of research is that one would be looking for mechanisms that are likely being kept hidden intentionally.

[27] In some cases over our sample period, the presidents of governing political parties were themselves federal ministers.

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Annex 1: Table 5: Districts having lost a minister over our sample period due to his/her resignation for a reason not related to "the fiscal transfer game"

Year	Electoral district	Minister	Exogenous reason
1996	Verviers	Wathelet Sr	Appointed judge at European Court of Justice
1998	Kortrijk-Roeselare-Tielt	Declerck	Resigned after escape of serial murderer and child rapist
1998	Veurne-Diksmuide-Ieper-Oostende	Vandelanotte	Resigned after escape of serial murderer and child rapist
1999	Antwerpen	Colla	Resigned after outbreak of food scare
1999	Limburg	Pinxten	Resigned after outbreak of food scare
2003	Brussel-Halle-Vilvoorde	Durant	Resigned after coalition conflict about airplane noise over Brussels
2003	Leuven	Aelvoet	Resigned after coalition conflict about arms deal with Nepal
2009	Aalst-Oudenaarde	De Padt	Demoted by his party to the position of "government commissioner" without a reason being known
2009	Limburg	Dewael	Resigned after allegations of influencing a civil service appointment
2009	Limburg	Vandeurzen	Resigned after the collapse of a large Belgian bank (Fortis)
2009	Mechelen-Turnhout	Vervotte	Resigned after the collapse of a large Belgian bank (Fortis)
2009	Veurne-Diksmuide-Ieper-Oostende	Leterme	Resigned after the collapse of a large Belgian bank (Fortis)

Source: Wikipedia; Federal Parliament of Belgium

Annex 2: Table 6: Number of extra ministers per district due to district merger over our sample period

Year	New electoral district	Old electoral district	Number of extra ministers due to merger	Population share of old in new district	Weighed number of extra ministers due to merger
2003	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2003	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	3	0.30	0.90
2003	Oost-Vlaanderen	Aalst-Oudenaarde	4	0.28	1.10
2003	West-Vlaanderen	Brugge	1	0.24	0.24
2003	West-Vlaanderen	Kortrijk-Roeselare-Tielt	1	0.45	0.45
2003	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	0	0.31	0.00
2003	Hainaut	Mons-Soignies	0	0.33	0.00
2003	Hainaut	Tournai-Ath-Mouscron	0	0.23	0.00
2003	Hainaut	Charleroi-Thuin	0	0.44	0.00
2003	Liège	Liège	1	0.57	0.57
2003	Liège	Huy-Waremme	2	0.17	0.34
2003	Liège	Verviers	3	0.26	0.79
2004	Oost-Vlaanderen	Gent-Eeklo	0	0.42	0.00
2004	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	3	0.30	0.90
2004	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2004	West-Vlaanderen	Brugge	1	0.24	0.24
2004	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2004	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	1	0.31	0.31
2004	Hainaut	Mons-Soignies	2	0.33	0.66
2004	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2004	Hainaut	Charleroi-Thuin	1	0.44	0.44
2004	Liège	Liège	1	0.57	0.57
2004	Liège	Huy-Waremme	2	0.17	0.34
2004	Liège	Verviers	3	0.26	0.79
2005	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2005	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	2	0.30	0.60
2005	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2005	West-Vlaanderen	Brugge	1	0.24	0.24
2005	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2005	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	1	0.31	0.31
2005	Hainaut	Mons-Soignies	2	0.33	0.66
2005	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2005	Hainaut	Charleroi-Thuin	1	0.44	0.44
2005	Liège	Liège	1	0.57	0.57
2005	Liège	Huy-Waremme	1	0.17	0.17
2005	Liège	Verviers	2	0.26	0.52
2006	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2006	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	2	0.30	0.60
2006	Oost-Vlaanderen	Aalst-Oudenaarde	3	0.27	0.82
2006	West-Vlaanderen	Brugge	0	0.24	0.00
2006	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2006	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	2	0.31	0.63
2006	Hainaut	Mons-Soignies	2	0.33	0.66
2006	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2006	Hainaut	Charleroi-Thuin	1	0.44	0.44
2006	Liège	Liège	1	0.57	0.57
2006	Liège	Huy-Waremme	1	0.17	0.17
2006	Liège	Verviers	2	0.26	0.53
2007	Oost-Vlaanderen	Gent-Eeklo	1	0.42	0.42
2007	Oost-Vlaanderen	Sint-Niklaas-Dendermonde	1	0.30	0.30
2007	Oost-Vlaanderen	Aalst-Oudenaarde	2	0.28	0.55
2007	West-Vlaanderen	Brugge	0	0.24	0.00
2007	West-Vlaanderen	Kortrijk-Roeselare-Tielt	2	0.45	0.89
2007	West-Vlaanderen	Veurne-Diksmuide-Ieper-Oostende	2	0.32	0.63
2007	Hainaut	Mons-Soignies	2	0.33	0.66
2007	Hainaut	Tournai-Ath-Mouscron	1	0.23	0.23
2007	Hainaut	Charleroi-Thuin	1	0.44	0.44
2007	Liège	Liège	1	0.57	0.57
2007	Liège	Huy-Waremme	1	0.17	0.17
2007	Liège	Verviers	2	0.26	0.52

Source: Wikipedia, Federal Parliament of Belgium

Annex 3: Federal electoral districts before and after the 2002 reform

Source: Hooghe e.a. (2006)